

SCIENCE

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THE TIME-RELATIONS OF MENTAL PHENOMENA.

[Continued from p. 101.]

Conditions Affecting Simple Re-action Times.

REFERRING to the accompanying table for a general view of the time-relations of simple re-actions, we may pass at once to the topic of greatest interest, viz., the influences by which they are quickened or retarded. These may be considered as (A) objective, or affecting the conditions of the experiment, and (B) subjective, affecting the attitude of the re-actor.

(A, 1) *The Nature of the Impression.* The distinctive

necessitating a precise accommodation, — a condition not always supplied in the above experiments. This view is strengthened by the shortening in the re-action time (by 36σ for Exner, by 24σ for v. Wittich) when the eye is stimulated electrically. In re-acting to a temperature sensation, care must be taken not to re-act to the sensation of contact with the skin. Where this has been done it has been found that the re-action to the sensations of temperature is longer than to contact, and that the re-action to heat is longer than to cold. Thus, Vintschgau and Steinach re-act to a pressure on various points of the head in 109σ , to a sensation of cold

Table of Simple Re-action Times.

No.	Nature of Sense-Impression.	Nature of Re-action.	Observer.	Time.	Remarks.
I	Visual (various kinds).....	Finger.	Average of many	185	
II	Tactile " ".....	"	" "	148	
III	Auditory " ".....	"	" "	139	
IV	Temperature (cold).....	"	Vintschgau } Av.	161	Average of all parts of body.
V	" (warm).....	"	Steinach } "	177	
VI	Light of intensity I.....	"	Berger & Cattell } Av.	210	
VII	" " II.....	"	" "	184	The intensity in terms of a common unit were as 7, 23, 123, 315, 1000, the two highest intensities not being determinable.
VIII	" " III.....	"	" "	174	
IX	" " IV.....	"	" "	170	
X	" " V.....	"	" "	169	
XI	" " VI.....	"	" "	156	
XII	" " VII.....	"	" "	148	
XIII	Touch (electric shock).....	"	Exner.	139	On the hand.
XIV	" " ".....	"	"	175	On the foot.
XV	Sound (low).....	"	Wundt.	175	With preparatory signal.
XVI	" (low).....	"	"	266	Without " "
XVII	Sound.....	"	Berger & Cattell } Av.	124	Preparatory signal at regular intervals.
XVIII	".....	"	" "	165	Preparatory signal irregularly varied within $\frac{1}{2}$ seconds.
XIX	Sound { Average of weak and loud	"	Wundt.	121	Intensity of sound known.
XX	" { sound.....	"	"	203	Intensity of sound unknown.
XXI	Sound.....	"	Münsterberg.	162	Attention directed to sensation.
XXII	".....	"	"	130	" " movement.
XXIII	Touch (electric shock in forearm).	By opening and closing jaw.	Av. Orchanaky.	155	Normal.
XXIV	" " " ".....	"	"	105	8 minutes after taking 60 ccm. of rum.
XXV	" " " ".....	"	"	225	30 " " " " " "

characteristics of a simple re-action being in the attitude of the re-actor, it would seem that its time could be little affected by the nature of the impression. The motor signaling process is the same, the connection between the impression and movement is about equally artificial in all cases, so that the chief variability must be in the time needed for receiving the impression. For the different senses this time is different. Taking the general average of all the observations accessible to me, I find, for hearing, 138σ ; for touch, 148σ ; for sight, 185σ . This order is quite constant with the different observers, the long time of visual re-actions being referable to the long inertia period of that sense, as well as to the small perceptive area of the retina,

in 135σ , of heat in 146σ , similar values for various points of the hand being 121σ , 188σ , 209σ . The researches of Goldscheider agree with these in the main, but make the difference between the re action times to heat and to cold much greater. The senses of taste and smell clearly illustrate the effect of the kind of stimulation, for here the relative inaccessibility of the sense organs and the slowly acting chemical processes involved lead to a long re-action time. Though experimentation is difficult and uncertain in these senses, we may cite for smell the results of Moldenhauer on the odors of various oils, centring about 300σ (oil of roses 273σ , camphor 321σ , musk 319σ , ether 255σ , etc.), and for taste, of Hönigschmied, who re-acts to various tastes on the tip of the

tongue in 182σ , though other subjects require about 300σ . On the back of the tongue the time is much longer, and it varies for different tastes, being longest for bitter, shortest for salt, and about equal for sweet and sour. Within the same sense the re-action time will vary according to the nature and place of the stimulus. The above cited differences for tastes and smells show this; and for different visual impressions, for different tones, for contact at different parts of the body, different results have been obtained, referable to slight variations in sensibility, length of nerve traversed, clearness of the impression, and the like. These minor differences are not easily established, but the following may be cited. Exner re-acts to an electric shock on the hand in 132σ , on the forehead in 137σ , on the foot in 175σ ; v. Wittich re-acts to a point on the back of the finger in a longer time than to one on the front, 144σ and 156σ , and regards the difference as due to a difference of sensibility. Hall and Kries clearly show that the re-action to a point looked at in indirect vision is longer than to one in direct vision, 195σ and 235σ , and find further differences according as the point is above or below, inside or outside, the retinal centre. A high tone is re-acted to more quickly than a low one, and so on.

It is easier to demonstrate the influence of (2) *the intensity of the stimulus*. Within limits, intense stimuli affect sense-organs more quickly than weak ones, and, roughly speaking, an increase in the intensity of the stimulus is concomitant with a decrease in the re-action time. According to Wundt, the noise of a hammer falling respectively from heights of 1, 4, 8, and 16 millimetres was re-acted to in 217σ , 146σ , 132σ , and 135σ , and the sound of a ball falling from heights of 2, 5, 25, and 55 centimetres in 176σ , 161σ , 159σ , and 94σ respectively. Exner varied the length and therefore the brilliancy of an electric spark from 0.5 to 7 millimetres and obtained a steadily decreasing re-action time of 158σ to 123σ . More complete are the observations of Berger and Cattell, who found that as the light increased from 7 to 23, to 123, to 315, to 1,000, and to two greater but not determinable degrees of intensity (as compared with a small unit of light), the re-action times fell (average of two observers) from 210σ to 184σ , to 174σ , to 170σ , to 169σ , to 156σ , to 148σ . For sound, as the ball fell from heights of 60, 160, 300, and 560 millimetres the re-action times were 151σ , 146σ , 127σ , and 123σ . For electrical touch excitations, re-actions to four grades of stimuli separated by equally perceptible differences were made (average of two observers) in 173σ , 159σ , 145σ , and 145σ . Wundt regards the difference in re-action times of the different senses as in part referable to differences in intensity, and, when re-acting to just perceptible intensities of sensation in various senses, finds about the same long time for each, 330σ .

3. *The Mode of Re-action*. The various movements by which we may signal that a sensation has been received may differ in the ease of their execution, in the length of nerve traversed, as well as in the naturalness of association with the impression. Such differences, however, seem to be small; when once the movement is understood and anticipated, the difference in the times of its execution is slight. Thus, Münsterberg found, in testing the re-action of each of the five fingers, that while at first the thumb and little finger re-acted more slowly than the others, after some practice the

times of all were substantially the same. Féré, however, has some results suggesting that the fingers making the strongest movements re-act in the shortest times. Very interesting, too, is the experiment of Ewald in which the stimulus, an electric shock, is given to the finger in the very key by which the re-action is signalled, the re-action consisting in the very natural movement of drawing the finger away. Under these circumstances he found a brief and constant time of 90σ . Both Vintschgau and Cattell have compared the time of re-acting by closing a key with the finger and by speaking a word, and find the vocal method the longer by about 16σ and 30σ respectively. Differences in re-acting from the two sides of the body have been observed by some, the right side showing the shorter re-action, but this difference can hardly be considered as constant. Orhansky has shown in one case that the movements of inhibition take about the same time as those of excitation of a muscle, and it would be possible to study the relative ease of various movements by this method. A practical example is furnished by the commands of military drill, the words, "carry," "present," etc., announcing the mode of re-action for the performance of which the following word, "arms," is the signal.

(B) We pass next to the more important *subjective factors*, referring in the main to the *expectation* and the *attention*. While nothing has been definitely said upon this point, the implication has been that the subject tries his utmost to re-act as quickly as possible, and that he knows the nature of the experiment. While the influences now to be discussed seem to be general in their effect, making the nervous system at one time a better and again a worse re-acting apparatus, they may, in certain respects, be subjected to a more definite analysis. We begin with (1) *the subject's fore-knowledge of what is to take place*. We may anticipate the outcome of experimentation on this point by formulating the law that the more definite the fore-knowledge of the subject the quicker the re-action. Apparently there is a process that must be gone through with in each re-action, and the better prepared the subject is for this,—that is, the more of this process gone through with before the giving of the stimulus,—the less of it falls within the measured interval. The precise nature of this process is a difficult and much discussed problem. It may be sufficient to note at present that the re action to a certain stimulus cannot but imply in some sense the distinction of that stimulus from the many others by which we are constantly surrounded. If the subject be re-acting to a visual impression, he will probably not press the key should a noise occur in the room or something accidentally come in contact with his hand. To re-act to a visual impression thus implies the distinction of that from other impressions. It implies the identification of the expected with the existing impression. Just as we recognize an appearance in the heavens or under the microscope more readily when we know where and what to look for, or as we immediately recognize an almost forgotten acquaintance when expecting him, though at a chance meeting we might have passed him without recognition, so we re-act to an impression most quickly when it is most definitely expected, with regard to its nature, its time and place of appearance, and the like. This expectation may be more or less specific, and an interesting series of experiments consist

in varying the fore-knowledge of the subject while still leaving it definite enough to call the result a simple re-action. (a) We may leave the precise *time* of the appearance of the stimulus undetermined. This may be done by experimenting with and without a preparatory signal, preceding the stimulus by a regular interval. Wundt re-acted to the sound of a ball falling from a height of 25 centimetres in 76σ with a preparatory signal, but in 253σ if no such signal preceded; to a ball falling five centimetres, in 175σ in the first case, and 266σ in the second. The time between the signal and stimulus is here regular, and the most favorable time seems to be about two seconds. Lange found the time with an interval of two seconds less than with one of one or three seconds. If the interval be irregularly varied within two seconds the effect is hardly noticeable, but if irregularly varied within fifteen seconds the time is increased (Cattell). With a normal re-action to sight of 149σ and to sound of 124σ , the re-action to sight with the interval varying within two seconds was 148σ ; when varying within fifteen seconds, to sight, 174σ , to sound, 165σ (average of two observers). (b) If the time and nature of the stimulus be known, but its *intensity* be varied, the time is increased. When re-acting to a uniform change between a feeble and a loud sound, the re-action time to the former was 127σ , to the latter, 116σ ; but when these changes were made in an irregular and unexpected manner the times were lengthened to 208σ and 198σ .

Similarly the attention may be prevented from being effectually directed to the making of the re-action by a variety of circumstances. Some of these we may group under the term (2) *distraction*. By a constant noise or other means we may be creating a stimulus to which the attention is involuntarily drawn, and thus withdrawn from the process of re-action. Wundt re-acted to a sound of mean intensity in 189σ , to a strong sound in 158σ , but when a disturbing sound was going on in the room these re-actions required 313σ and 203σ . On the other hand, with Cattell, when in good practice, so that the re-action became almost automatic, the effect of a disturbing sound both upon sight and sound re-actions was insignificant,—normal for sight, 149σ , with disturbing noise, 155σ ; normal for sound, 124σ , with disturbing noise, 124σ . It is quite probable that what acts as a disturbance to one person hardly affects another. In some individuals the re-action time seems to be extremely sensitive to any mental disturbance. One of Obersteiner's subjects, with an average re-action time of about 100σ , requires 142σ to react when music is heard, and another's re-action time is lengthened by 100σ when talking is going on in the room.

A more general and thorough form of distraction may be effected by imposing a task requiring distinct mental effort at the same time that the re-action is to take place. Thus Cattell attempted to add 17 consecutively to a series of numbers, and found that re-actions taken while this was going on were longer by 28σ (average of two observers). All such effects seem to be much more marked when the re action in question is new than when it has become familiar and partly automatic. The disturbance seems to act by delaying the association between stimulus and movement.

(3) We have now to notice a distinction which, though but recently brought to light (by N. Lange, 1888), is of fundamental importance. A re-action may be made in two ways. In the one form of re-action the attention is directed

to the expected impression: it is identified as the expected impression, and thereupon is initiated the impulse resulting in the re-acting movement. The several processes are performed serially, the attention being concentrated upon the sensory part of the process. In the other form of re-action the attention is directed to the movement: the impulse is ready, and is set off by the appearance of the signal almost automatically, the identification of the actual with the expected impression being omitted. The first is spoken of as the "complete" or "sensory" mode of re-action, the second as the "shortened" or "motor" form. In the experiments of Lange the simple sensory re-action time to a sound (average of three persons) was 227σ , motor 123σ ; to a visual impression (average of two persons), sensory 290σ , motor 113σ ; to a tactile impression (one person), sensory 213σ , motor 108σ . These differences, however, seem to be extreme. Münsterberg finds for sound, sensory 162σ , motor 120σ . A further characteristic of the motor form of re-action is that its average variation is smaller, i.e., the process is more regular; and that false re-actions occur, either anticipations of stimulus or re-actions to some accidental disturbance. The distinction becomes still more important when the re-action is not simple but complex, and we will return to it later. The distinction is important as aiding in the explanation of individual differences, as well as of the path of practice. The somewhat conflicting results obtained before this distinction was taken into account might very well be due to the fact that the one observer re-acted in the one way and the other in the other. Thus the re-action times of Kries and Auerbach are motor; for they are brief, false re-actions occur, and it is noted that the simple re-actions following re-actions involving distinctions were longer by 41σ and 31σ than before,—a change probably due to a return to a partially sensory mode of re-action. Again, there are doubtless transitional modes between the two, and there are reasons for believing that the path of practice is from the sensory to the motor form of re-action.

The influences that remain to be discussed may be considered under the heads of "practice," "fatigue," "individual differences," and "abnormal variations." (4) *Practice*. As just noticed, the effect of practice is intimately connected with the mode of re-action. It is noticed by almost all writers, but the extent to which it influences the time is very various. The observations make it probable that the effect of practice is most marked at first, and that when once the initial stages are over, the effect of continued practice is small. It is greatest in those persons whose time is longest at first, and seems most influential in acts that are complicated and lie somewhat beyond the realm of daily experience.

When the action is once thoroughly learned, an interval of disuse seems not to affect the time seriously. After not re-acting for three months, Cattell found no essential difference in the time. On the other hand, with some there is a slight newness on beginning each day's work, making the first re-actions of a series rather long (Trautscholdt).

(5) A similar statement may be made of *fatigue*: it has greatest effect upon the complicated, less thoroughly learned processes, and varies with the individual and the mode of re action. With an automatic simple process its effect is very slow to appear (Cattell). It may enter at any stage of the process, sensory, motor, or central; but the last seems to

be the most serious. It appears as a difficulty in keeping one's attention upon the experiment, and thus lengthens the time, and especially the average variation of the experiments. By fatigue is meant the fatigue brought about by the experimenting itself. The time is also affected by general fatigue preceding the experiment. Some individuals are extremely sensitive to influences of this kind.

(6) *Individual Variations.* The fact here to be investigated is the general one that different persons require different times for the performance of the same operations. The difficulty of drilling a company of men to act in concert, whether in military drill or otherwise, springs in part from this difference. It was from this point of view, too, that the time of mental processes was first studied. So long ago as 1795 Maskelyne, the astronomer royal, discharged his assistant because the latter recorded the transit of a star across the wire of the telescope half a second or more later than he himself. Some twenty-five years later Bessel, another astronomer, had his attention called to the point, and upon investigation established the fact that no two observers recorded such transits at precisely the same time. The difference in time between any two observers was usually expressed as an equation, and hence the term "personal equation," which, though strictly applicable only to the differences so found, has assumed a much wider meaning. The individual differences become greater as the process to be performed increases in complexity, and this explains in part why the personal equations as determined by the complicated eye and ear method were so large: with the simpler method of electrical record these differences are much reduced. Besides the differences due to practice and the mode of reaction, there are a large number of minor sources of variation, which as yet are not sufficiently understood to justify a correlation of quick or slow re-action times with definite individual qualities. We may, however, note (a) that the time is longer in children than in adults, as has been shown, amongst others, by Binet, who found that children from 3½ to 7 years re-acted in from 440σ to 66σ, when adults required but 140σ. In the very old the time is longer than in the prime of life. Under the influence of mental or physical fatigue, worry, or slight indisposition, the time has been increased. Obersteiner, Vintschgau, Goldscheider, and others, have incidentally observed these effects, showing an increase of 30σ to 40σ. These variations are related to others, shading over into the abnormal. Under this head may be considered (7) *the action of drugs and re-action times in the insane.* Several of the earlier experimenters made a few observations concerning the effect of drugs. Exner found quite a marked lengthening of the time after drinking wine. Vintschgau and Dietl found that the effect of coffee was to decrease and of morphium to increase the time for a considerable period. The more elaborate researches of Kraepelin show that the effect of amyl, ether, and chloroform is a sudden lengthening of the re-action times, reaching a maximum in a very few minutes, and followed by a rather long period of times slightly shorter than the normal. If a strong dose of the drug be used the lengthening is more considerable and the secondary shortening slighter. Thus Kraepelin, whose normal re-action was 183σ, after a strong inhalation of ether re-acted in 298σ, and in the period of shortening in 170σ; while with a light narcosis the maximum re-action

was 223σ, and the shortened re-action 150σ. The effect of alcohol, however, is a brief period of shortened times followed by a long period of lengthened times. This is also found by Orskowsky, who, with a normal re-action of 155σ, re-acts in 105σ eight minutes after taking a dose of alcohol, and in 225σ after thirty minutes. The observations of Warren do not yield equally positive results, but do not conflict with those of Kraepelin. Changes in the extent of the average variation have also been observed. On what psychological factors these differences depend it is difficult to say, but the subjective feelings accompanying the lengthened times are a difficulty in keeping the attention upon the matter in hand, and an unwillingness to exert one's self. The evidence afforded by the action of drugs upon these processes is important as indicating the dependence of the re-actions upon physiological conditions. A change of re-action times in insanity has been frequently observed, but the field for individual variation is here very large. It seems probable that in most forms of mental disease, and particularly in melancholia, there is a considerable lengthening of the re-action time, amounting in extreme cases to one-half or three-quarters of a second. In the excited forms of disease, such as mania, a shortening has been observed. Obersteiner cites a case of general paralysis in the incipient stages of which the time was 166σ, in a more advanced stage 281σ, in a most advanced stage 451σ. Stanley Hall has found a marked shortening of the time in the hypnotic condition, but his result is not corroborated by others.

Methods of Experimentation.

The chief requisite in these experiments is an apparatus for accurately measuring small intervals of time. The earliest method, still in use, records the vibrations of a tuning-fork upon the quickly-moving smoked surface of a rotating drum, and beneath this the moment of giving the signal and making the response. If a fork making one hundred vibrations per second be used, whole hundredths can be directly counted and smaller fractions estimated. Wundt has constructed a more accurate and specialized instrument in which a fork making five hundred vibrations per second is used. A very much simplified form of apparatus has been devised by Obersteiner, in which the slide holding the record is moved by hand, and the movement of re-action draws the fork off the record; and by Bowditch, in which the fork itself carries the record, and the signal and re-action are indicated by a shifting of the writing point. In the astronomical records clock-work takes the place of a tuning-fork. The objection to these methods is that they necessitate tedious counting of curves. If the rate of the rotating-apparatus is very uniform and frequently tested, one may substitute measuring for counting, but the most convenient apparatus for the purpose is the Hipp chronoscope. This instrument contains a fine clock-work, set in motion by releasing a spring and running for about half a minute. The hands of the two dials, the one indicating tenths and the other thousandths of a second, do not move until drawn away from a set of cogs by the opening or closure of a magnetic circuit, and are stopped again in the same way. By making the usual arrangement whereby the production of the stimulus sets the hands in motion and the re-acting movement brings them to a stand-still, we can read off directly the interval of the re-action time. Unless we can afford to sacrifice accuracy for convenience, a means of controlling the chronoscope is indispensable. This may be done by timing the fall of a ball from a given height and comparing it with theoretical time. In the apparatus for this purpose supplied with the chronoscope the ball is mechanically released, and the mode of making the circuit is equally defective, so that

the error of the control apparatus is probably greater than that of the chronoscope. To obviate this difficulty I make use of a ball held in position by a magnet, and falling from any height up to seven feet, upon the arm of a well-balanced lever, thus securing an instantaneous release. By setting the magnet and ball at different distances we are also enabled to decide whether the error is absolute or relative. It is here necessary to break the current by which the ball falls, and to make the current by which the chronoscope starts at the same moment; this is effected by a key specially devised for the purpose. The chronoscope possesses a regulation for alternating its rate when too slow or too fast, but I find it most convenient to make sparing use of this, and apply a correction for each day's determinations as found with the "fall apparatus." Another form of control makes use of a falling hammer, the record being also made with a tuning-fork. A recent device of Ewald combines the two methods by mechanically counting the vibrations of a tuning-fork: a delicate armature is drawn to and released by a magnet with each vibration of the fork, moving the hand of a dial over one of its divisions as it does so. The fork is vibrating constantly, but the making of the signal sends the current into the "interruption-counter," while the re-acting movement again diverts the current away from it. It will record at the rate of one hundred per second. Galton has constructed for ordinary uses a machine in which the signal is given by the release of a rod or pendulum, and the re-acting movement mechanically arrests the fall or swing, a scale of interval being determined for the apparatus empirically. Sanford has devised a simple but not expeditious chronoscope, in which the signal and the response separately set in motion two pendulums of slightly different periods, the re-acting interval being calculated from the number of oscillations occurring before the two are again in unison.

The methods of indicating the moment at which the signal appeared and the moment at which the re-acting movement was made are simple. When the record is written on a rotating surface, a point connected with a magnet, and writing a straight line beneath the vibrations of the fork, writes that line at a different level when the signal is given, and returns to the same level when the re-acting movement is made; or the tuning-fork itself may be made to write at a different level during the interval measured. The arrangement by which the level is changed on the record, or the hands are set in motion in the chronoscope at the same instant that the stimulus appears, is equally simple. For sound, the noise of the key by which the circuit is made is generally sufficient, or other sound may be produced by bodies falling upon various surfaces and thereby opening or closing a key. For sight, the impression to which a re-action is to be made may be concealed behind a screen, and the drawing away of this screen at the same time makes or breaks an electric circuit. Frequently the re-actor sits in the dark, and the impression becomes visible only when an electric spark appears, or the spark itself may be the stimulus. For touch, temperature, and taste, a typical device is that of Vintschgau, in which the end of a rod touches the sensitive surface, and the pressure so exerted makes a contact with a delicate metallic blade inserted in the same apparatus. For smell, the movement by which the odor is set free is similarly utilized. The re-acting movement is usually that of pressing an ordinary telegraphic key. Devices have been constructed by which movements of the foot, of the jaw, of the voice and lip, may be similarly noted. For more detailed descriptions consult the references under this head at the end.

JOSEPH JASTROW.

[To be continued.]

In the *Atlantic* for September Mr. Justin Winsor considers the "Perils of Historical Narrative," Mr. J. Franklin Jameson contributes a paper on "Modern European Historiography," and Mr. Fiske adds an article on the "Disasters of 1780." These three papers furnish the solid reading of the number.

PROFESSOR A. GRAHAM BELL'S STUDIES ON THE DEAF.

In the year 1888 the Royal Commission appointed by the British Government to inquire into the question of the care and education of the deaf called to their aid Dr. E. M. Galaudet, the distinguished president of the National College at Washington, and later Professor A. Graham Bell, whose long interest in the deaf qualified him more than any other public man in America, outside those directly engaged in the work of instruction, to speak with some degree of authority on the questions presented to him. The information presented by Professor Bell has been published in pamphlet form, entitled "Facts and Opinions," and contains a great variety of facts concerning visible speech, heredity, day schools, articulation, and kindred subjects. The Royal Commission has recently completed its investigations, and reported to Parliament the results of its work. I mention the report right here, to draw attention to another of those singular conclusions which have characterized the opinions of men of unquestioned learning and intelligence, when undertaking to speak officially concerning the deaf. In paragraph 398 of this report we find this extraordinary statement, "The want of exercise of the lungs and throat on the part of pupils taught by the manual method is apt to produce chilblains." Two members of the Commission had the good sense to dissent from this paragraph, and officially to pronounce it "quite absurd."

It is impossible within the limits of this article to discuss *seriatim* the several subjects upon which Professor Bell has addressed the public, and I am therefore compelled to make a selection from those studies with which his name has become most closely associated, and from these it will be easily possible to infer the value of what he has done for the deaf. It is also necessary to add, that, in the friendly contention for methods aroused by Professor Bell's long indictment of our American schools, there is on our part a ready recognition of the honesty and zeal which has inspired him; and if we speak plainly on the studies which he has given to the public, we ask for ourselves a recognition of the same sincerity, something of that same chevalier spirit with which he has carried his lance against us.

The first measure for the education of the deaf with which Professor Bell became identified was "visible speech." This is a system of universal alphabetics, originated by A. Melville Bell, and was introduced into the United States nearly twenty years ago. The first exposition of this system of vocal physiology in the city of Boston created quite a sensation in literary circles. The extraordinary statement, during the first few months of trial, that "adult deaf-mutes had acquired all the sounds of the English language in ten lessons" (Report Massachusetts Board of Education, 1871-1872), drew, at once, the attention of all those interested in the deaf to this new device. From the report already referred to, and from that of the succeeding year, we find that "the effects produced by this new system are in the highest degree remarkable—even miraculous;" and again, "Perfect and pleasing articulation is certain." To this last claim, it is enough to say that to day there is not a reputable teacher in the United States who makes any approximation to so rash a claim. In view of all that was claimed for visible speech, it is not surprising that it soon became among us a

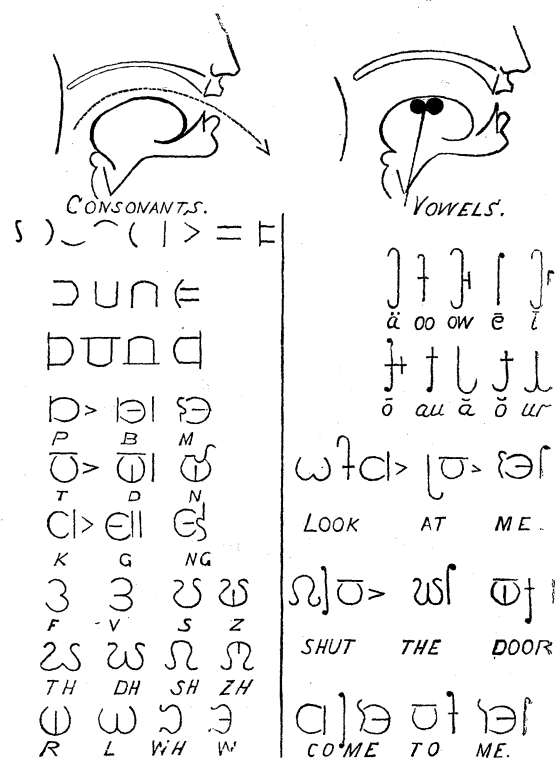
sort of craze, and that teachers in the art were everywhere in demand. The Clarke Institution at Northampton and the American Asylum at Hartford were fortunate in securing the personal services of Professor Bell, to give special instruction in introducing the Bell method. The number of adherents increased rapidly, and at Worcester, Northampton, and Boston enthusiasts in the cause held conventions to promote the new evangel to the deaf. The sweet optimism of the first disciples was delightful. But this system, from which so much was expected, of which we believed so much and knew so little, has, after nearly twenty years, ended with the quantities inverted. For all purposes of articulation, if not already dead, it is certainly moribund. It has been tried and dropped in the following schools: the American Asylum at Hartford, the Clarke Institution at Northampton, Pennsylvania State Institution at Philadelphia, Western Pennsylvania Institution, Wisconsin Institution, Minnesota, Missouri, Ohio, Kansas, and Tennessee State schools, and at Belleville, Ontario. It is true that in the statistics presented to the Royal Commission, visible speech is still made to hold a place in our American schools, but a glance at some of the remarks accompanying the affirmative answers found in the table on page 2 of "Facts and Opinions" reveals the fact that some of the questions concerning visible speech would have been more correctly answered in the negative. "It is used," says Dr. Gillette of Illinois, "to a very limited extent. It is of advantage to teachers, as it enables them to comprehend physiological facts involved in speech; but for pupils, while it is thus helpful, it requires an amount of time and labor to acquire that can be better improved by the use of diacritical marks." The principal of the school in Florida answers "No" to the question concerning visible speech, but because he uses a method of line-writing adopted by Professor Bell, he is classed among those endorsing the Bell system. Even among those schools where the symbols are still used there is a wide difference of opinion as to its utility. The principal of the Kendall School, Washington, reports that it is used only in the initiatory steps, while in the Horace Mann School, Boston, it is used only with the older pupils. It is impossible to understand the difficulties involved in this system of "visible speech" without some examination of the characters to be studied, and they are here submitted.

These characters, deaf children of eight and nine years of age, coming to school for the first time, never having heard a word of sound in their lives, with minds a complete blank to all the world of sonal thought, were obliged to master and at the same time begin the work of reading and writing English. The wonder is not that the best schools have given it up, but that it was permitted to hold its place among us so long.

The former advocates of visible speech now fall back upon the consolatory reflection, that, although impracticable for children, yet every teacher ought to have a knowledge of it; but unfortunately our best authorities on vocal physiology are not agreed even on that point. There is no one better qualified to speak on questions of this character than Mr. David Greenberger, principal of the New York City School for Improved Instruction, and this is what he says of the Bell symbols: "These hieroglyphs could no more assist a mute in his attempts at vocal utterance, than the signs of the

Zodiac." The directive power of the symbols, upon which so much stress was laid, Principal Greenberger declares to be a myth. Every one is willing to admit that a knowledge of vocal physiology is an absolute pre-requisite to all successful teaching of articulation, but this attainment is in no way contingent upon the study of visible speech. If there are those who care to study phonetics, freighted with an alphabet of visual mnemonics, they cannot do better than take up the system propounded by Professor Bell, but that there is any logical connection between the two things, is a contention that has not a foot of ground to stand upon.

The second distinctive educational device which Professor Bell advocated for the deaf was a system of line-writing, a form of visible speech put into short-hand. An experimental school was formed at Washington by the Professor, and at the Convention held in New York City in 1884 line-writing was



explained by one of his teachers, and great things claimed for it. Line-writing was, in fact, a system of short-hand. It is possible that those versed in visible speech may find some connection between the uncials of that system and the strokes used in line-writing. It must be set down to the credit of the six hundred teachers of the deaf in the United States, that, with the exception of one case already noted, all have united in rejecting the use of stenography for young deaf-mutes. The experimental school lived a very transitory life; and in the literature of teaching, only one writer has ventured to utter a word on this last expedient advocated, but not originated, by our enthusiastic friend.

On the question of heredity, Professor Bell has collected a large number of interesting facts. Men may differ as to some of the conclusions drawn from the statistics he has gathered, but there can be no question in regard to the industry and care with which he has devoted himself to this particular line of inquiry. So thoroughly has the danger of deaf-mute offspring from deaf-mute marriages been preached, that

wherever I have heard the question discussed among the general public, there is the greatest surprise expressed that the deaf should have hearing children. The discussions of the past few years have left upon the minds of many intelligent people the impression that the marriages of deaf-mutes are the prolific cause of the increasing number of deaf people in the United States. In considering the sociology of the deaf, it is necessary to bear in mind the following facts:—

The Clarke Institution at Northampton, Mass., opened in 1867. There have been several marriages among the pupils of the school, but none of them have had deaf-mute offspring.

There have been seventeen marriages of the pupils of the Horace Mann School, Boston, but none of the children of these marriages are deaf.

Principal Hutton of Halifax reports thirty marriages of pupils from his school, but only in one case do the children share the infirmity of the parents.

Mr. Mathison, Superintendent of the Bellville Institution, Ontario, says: "Six hundred and sixty-one children have attended, or are in attendance, at this school, and from the records I find that not a single parent of these children is deaf." The principal of the Minnesota School, after an experience of twenty years, and those of California, Alabama, and Mississippi, report a similar state of things in their respective states. But perhaps the most satisfactory statistics concerning the deaf are found in the reports of the Irish Commissioners. In 1881 these commissioners report as follows: "An inquiry having been carried out in the censuses of 1851, 1861, and 1871 as to the children of congenital deaf-mutes, and the result being in each case of a negative character, it was not considered necessary to repeat this investigation, as it appears evident that deafness and dumbness in the parents have no influence in propagating the defect."

In the census of 1871, a minute investigation was made respecting the marriage-state of congenital deaf-mutes, and from 115 unions there were found to be 315 children, of which number only three were deaf. Compare now this result with the number of deaf children from consanguineous marriages, and we find 141 cases of congenital deafness from the inter-marriage in 85 instances of first cousins; in 63 instances of the marriage of second cousins there were 100 deaf children; there were in all 324 cases of deafness from 194 intermarriages among relatives. One striking instance will illustrate the fact that consanguinity in the parents is responsible for a large percentage of deafness.

The Irish Commissioners report that No. 6 in their returns consisted of a family of five children whose parents were second cousins, two of the five children were born deaf. The father married a second time, but this wife was not related to him, and the six children resulting from the union were perfectly developed in all their faculties (Annals, Vol. xxx., No. I., p. 51). In the discussion on the papers read by Professor Bell before some of our scientific associations, he expressed the opinion that consanguineous marriages were not so productive of deaf-mute offspring as people generally supposed. How accurate this opinion is, the facts already quoted will show. No one claims that the mere fact of relationship is in itself a cause for deaf-mute offspring. It is quite probable that some lurking disease, some hereditary taint, becomes intensified in the offspring of consanguineous parents, and the children in consequence become deaf, though

why it should affect the hearing is a problem no one yet has been able to solve.

Professor Bell's indictment of the sign-language has been completely answered by Dr. Williams, the principal of the American Asylum at Hartford. By thirty-two cases from schools where signs are prohibited, he has shown that the pupils taught there have all the peculiarities which mark the diction of children educated by means of the sign-language.

Permit me, in conclusion, to cite an instance which indicates a tendency to be guarded against on the part of the Professor, a tendency, too, which has marked the literature of our deaf-mute press in their animadversions on the published addresses which he has given to the public, but, wherever found, the practice is indefensible.

In his address at the Gallaudet Centennial in Philadelphia, the sweeping statement was made that there were 15,000 children of school age not receiving any education. This statement was at once challenged, but the Professor quoted in his defence statistics given in advance by F. D. Wines of Illinois. There would be some justification for this error, if Mr. Wines had not publicly stated, before Professor Bell at the convention in New York in 1884, that the number of children of school age not under instruction was 5,000. The number of deaf-mutes in the United States at the time of this charge was 33,000. Of these, 15,000 were under twenty years of age. The number of children between ten and twenty years of age was 10,000, and of these 6,900 had been under instruction during the period here considered, so that the claim of 15,000 children not receiving any instruction was very wide of the mark. (See Report to British Government, p. 51.)

W. G. JENKINS.

NOTES AND NEWS.

THE railway tunnel under the St. Clair River, between Port Huron, Mich., and Sarnia, Ont., is rapidly approaching completion. Communication between the headings from the opposite sides of the river was effected on Aug. 25. This tunnel is considered the greatest engineering work of the kind in this country.

—On Saturday, Aug. 23, the remains of John Ericsson, the eminent engineer and inventor, were removed from the vault where they were deposited at the time of his death, in March, 1889, taken aboard the United States man-of-war Baltimore, amid imposing ceremonies, and are now on the way to Sweden, the place of his nativity.

—Some habits of crocodiles have been lately described by M. Voeltzkow. Travelling in Wituland, says *Nature*, he obtained in January last seventy-nine new-laid eggs of the animal, from a nest which was five or six paces from the bank of the Wagogona, a tributary of the Ooi. The spot had been cleared of plants in a circle of about six paces diameter, apparently by the crocodile having wheeled round several times. Here and there a few branches had been laid, but there was no nest-building proper. The so-called nest lay almost quite open to the sun (only a couple of poor bushes at one part). The eggs lay in four pits, dug in the hard, dry ground, about two feet obliquely down. Including eggs broken in digging out, the total seems to have been eighty-five to ninety. According to the natives, the crocodile, having selected and prepared a spot, makes a pit in it that day, and lays about twenty to twenty-five eggs in it, which it covers with earth. Next day it makes a second pit, and so on. From the commencement it remains in the nest, and it sleeps there till the hatching of the young, which appear in about two months, when the heavy rain period sets in. The egg-laying occurs only once in the year, about the end of January or beginning of February. The animal, which M. Voeltzkow disturbed, and saw drop into the water, seemed to be the *Crocodilus vulgaris* so common in East Africa.

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Attention is called to the "Wants" column. All are invited to use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

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THE RELATIONS OF THE STATE AND NATIONAL GEOLOGICAL SURVEYS TO EACH OTHER AND TO THE GEOLOGISTS OF THE COUNTRY.¹

INVESTIGATIONS undertaken in the course of instruction are often carried on in college laboratories, but it is rarely that individuals or institutions, other than the State and National surveys, have undertaken extensive and expensive geological operations. We must except, of course, in this statement those of an economic nature.

The reason of this is that individuals seldom, and commercial organizations never, devote their means to purely scientific investigations; and inasmuch as these investigations require large sums of money, and as they are conducted largely with a view to increasing human knowledge, the expense of them must be borne by the public treasury.

With our official organizations, most of the working geologists of the country, excepting those called consulting geologists, are connected either as salaried assistants, permanent or temporary, or as volunteers. But these organizations carry on their work independent of each other, indeed, without any regard to one another's existence, while individual investigators go each his own way, pretty much as if he had the whole world of geology to himself.

¹ Abstract of an address before the Section of Geology and Geography of the American Association for the Advancement of Science, at Indianapolis, Ind., on Aug. 20, 1890, by John C. Branner, vice-president of the section.

Practically the United States Geological Survey has *carte blanche* to carry on geologic investigations over the whole territory of the United States, and in every branch of scientific work directly related to geology, such as geography, topography, paleontology, physics, chemistry, and statistics.

Now, over this same area, though limited to the States carrying them on, we have our several State geological surveys; while private individuals, educational institutions, scientific societies, and commercial corporations are at liberty to carry on such investigations as they see fit, and all regardless of each other.

In the earlier work carried on by the Federal Government, however, the various Territories were the specified areas to which the National surveys were confined, and now that the whole area of the United States is open to this work a broad-minded and co-operative direction seeks to aid and strengthen the State organizations instead of antagonizing or annihilating them.

But I wish to emphasize the fact that the classification of the geologists of the country, the work within the domain of the National survey, the work within the domain of the State surveys, and that which can be or will be accomplished by private institutions, corporations, or individuals, demand that there should be some definite and better organized co-operation or co-ordination in all this work and among all these men.

The statement has been made that the United States Survey does co-operate with nearly every State survey in the country, but the fact is that the National survey does not know what the State surveys are doing except in a very general way, and that the State surveys know but little or nothing of what the National survey is doing, except, perhaps, as it may happen to be picked up in private conversations and in private correspondence between personal friends.

Please bear in mind that this is not intended as reflecting upon the Director of the United States Survey; co-operation can only exist by the common consent of all the parties concerned, and it is quite as much the fault of the State surveys that there is no such co-operation as it is that of the United States Survey.

What I have to say, however, refers to the internal arrangements and the working of geologists as affected by our own bearing toward the official surveys, toward each other, and toward the science, rather than toward official relations and toward legislation. For these are not matters to be fixed by laws: laws would interfere with that freedom of movement that gives health, vigor, and activity to our scientific bodies and to our scientific men; they can only be determined by common consent and by usage.

The United States Geological Survey stands at the official head of all our surveys and of all our geological work. National work encourages and stimulates State work, and State work re-acts in favor of national work, and both stimulate private enterprise and investigation. The return from all this no man can measure, for it is both material and intellectual, and in both these senses it is felt in every nook and corner of the land.

The National survey is thus doing a work that no other institution can do, and it is able to maintain an organization of geologists that no other institution could maintain. For nowhere, in no country, is there, and at no time has there been, a corps of working geologists superior to that of our present National survey—a body of geologists of which every scientific man, and, indeed, every citizen of this country, may well be proud.

Having no connection with that organization, either present or prospective, I feel at liberty to express this a frank, disinterested and independent judgment.

With its splendid equipment of men and means, what can the National survey best do, and best leave to State surveys and to private enterprise? The question is not asked as implying that the officers of that body are not perfectly competent to decide these matters, but because we feel that a more effectual co-operation can be brought about to the great advantage of every one concerned. So long as more than one organization must occupy the same field, some understanding can certainly be arrived at that will prevent the duplication of work and the waste of energy and of funds. The appliances, libraries, laboratories, equipments, and the large number of special assistants required by a National survey, are quite beyond the means of our modest State surveys.

The great size of our country, the wide sweeping character of its general geologic structure, and the limits placed by civil boundaries on State work, must throw most of the important general questions into the hands of the National survey. Local details can and should be worked out by the State surveys, and these results should be placed as soon as possible at the disposal of the specialists of the National survey. It is self-evident that problems that can be solved only after a wide experience and acquaintance with the whole country can not be satisfactorily undertaken by the State surveys, but that they must be solved by the larger and stronger organization.

There are certain classes of work that, of necessity, fall upon a National rather than upon the State surveys; such are triangulation, precise levels, topography, paleontologic work, almost all investigations falling under the head of what is usually known as pure science, and all those investigations requiring much time and labor and money and many specialists. The reasons why State surveys can not do work of this class are not far to seek. The men with whom the National survey has to deal are our broadest minded statesmen,—men who comprehend the scope and importance of purely scientific work, while, as a rule, State legislators look to immediate and what they call practical results. Such men can not be convinced of the importance of any work that looks not to the immediate material prosperity of the State, while they are but little concerned, as a rule, with the intellectual income from it.

It is entirely beyond the means of any State survey to make a topographic map of the entire area of the State: the best it can do is to select a few typical areas and map those. But maps are absolutely essential to satisfactory geological work, and map making has come to consume a constantly increasing share of the money appropriated for geological surveys, both State and National. The National survey, however, having large appropriations for topographic work, and contemplating as it does the mapping of the entire area of the United States, ought to do this work.

That these maps must meet various demands, and must therefore be constructed with varying degrees of accuracy and detail, every one will admit. As a matter of fact, however, the maps made are usually, as they should be, parts of a plan, and upon a scale for mapping the whole of the United States. This plan and scale may be perfect for that particular purpose, but it often happens that neither the plan nor the map is adapted to the purposes of the State surveys. And certainly nothing can be plainer than that the maps made by a geological survey ought to be available for geological work, or that, failing to meet the demands of geology, there is no geological excuse or reason for their existence.

Geodetic work can not be carried on by the States, because States are but small parts of and furnish but few points in geodetic questions. European countries have even been obliged to unify their work. In the United States work of this character must be left to some institution of the general government. That paleontologic work should be relegated to the National survey seems to me scarcely to admit of question.

It might be urged against these reasons that the States of Illinois and New York afford striking examples of the fact that States may and can and do carry on a high grade of paleontologic work. But it should be remembered that the conditions under which these excellent results have been obtained have passed or are about to pass away. For, while the States of Illinois and New York have grown in wealth and intelligence since their surveys were begun, the Legislatures of those States could not to-day be induced to take up and carry forward works of so purely a scientific nature; and if those States could have seen the end from the beginning it may well be doubted whether they would have undertaken the great paleontologic investigations carried on so long and so successfully by Hall and by Worthen.

Another point which I must insist upon is that it is the place of a State geological survey to do what is wanted in the State, and as a rule economic results are wanted. The people are entitled to what they pay for. Not that the survey must go on every wild-goose chase suggested and examine every prospect and claim in the country, but the problems which the people wish to have solved should be solved if they can be solved.

These very demands define the work of the State surveys, and separate it pretty sharply from that of the Government survey. If we are to be perfectly honest with ourselves, we must confess that State surveys have, as a rule, failed to do what the people have expected of them, and one of the principal reasons for these failures is that the geologists have not had the counsel and the co-operation of a National survey. The geologists who have encouraged the making of appropriations for the work have invariably held out the hope that these surveys would be devoted to economic geology, while members of Legislatures who have supported such bills have invariably done so in the expectation that they would do something of direct economic importance. But there are but few exceptions to the rule that these State appropriations have been devoted to paleontologic problems and to pure science, while economic problems have been entirely lost sight of.

These economic problems, or such of them, or rather, perhaps, such phases of them as can safely be dealt with by a State, should be the special province of the State surveys, while the broader questions which can be satisfactorily studied and safely discussed only over wide areas should be left to the National survey.

It is true that economic and purely scientific problems cannot be entirely separated, and there is no necessity that they should be, but geologic work may give preference or prominence to one or the other phase of the question as the case may demand. I have said that economic problems should, in so far as possible, be left to the States. There are cases, however, in which this cannot be done, for there are often those which, requiring study over a wider area, cannot be solved in a single State. These should be studied in part or entirely, as the case may demand, by the National survey.

It seems plain, in so far as the relations between the National and the State surveys are concerned, that the National survey should leave all that it can safely leave to private enterprise and to State surveys, and it should deal with those problems which State surveys and individuals will not or can not satisfactorily deal with.

It is my opinion, also, that the National survey, being better informed of what is going on in the way of geologic work than the State geologists, and being in every respect the strongest of our organizations, should hold out a helping hand to the State surveys, and from their wider and more valuable experience, give advice and encouragement to State work. In this way State aid to scientific work would be encouraged and the National survey would widen its helpful influence.

It goes without saying that State and National surveys should not ride rough shod over each other just because there is no law to prevent their duplicating each other's work or their doing work that will interfere with each other's plans or efficiency. It would be easy for a Government survey to discredit and embarrass a State survey to such a point that the State would put a stop to its own work. Fortunately, our National survey has been conducted rather with a view to aiding the State surveys. But this aid can be made much more effectual than it ever has been, and I have no doubt it will be made so whenever we are all ready for such co-operation.

What must a man's feelings be when he brings his contribution, to find that it is in the wrong place, or that it is not wanted. Mistakes of this sort are constantly being committed in geologic work, and in abundance too, all because we have no recognized directing head for the work done outside of the United States Geological Survey.

The bulk of geologic literature must yearly become greater, and unless it becomes at the same time better, we must expect a day to arrive when geologists may well stand appalled before it. Much of the literature is practically worthless; it is an encumbrance rather than a help to the progress of science, and we should feel grateful to any method that would deliver us and geology from an evil which is coming to be a more and more serious one.

In one of the States in which the United States Survey has been doing topographic work, an area of 3,000 square miles that had already been surveyed had to be remapped by the State survey to meet its own demands. Here, I think, no one will have any diffi-

culty in understanding the necessity of co-operation between the State and the National surveys.

Take as another example the chemical analyses made for geological purposes. The chemists of State and National surveys have thrown upon them a vast amount of heterogeneous work, while but little or no time is left them for original investigations. A great many of their analyses are duplicated elsewhere, or may be duplicated in any number of laboratories, so that investigations that might otherwise have been possible are prevented, and both chemistry and geology are hindered.

The errors made by geologists not connected with the surveys are mainly due to haste, or, in other words, to expression of opinion based upon too limited observations. But only limited observations are possible to men of limited time for the work, and limited means to work with, a limited area to work in, limited acquaintance with field geologists, and limited opportunities for publication. There are many young geologists and men of but little experience—amateurs—whose efforts are not so directed as to be of as much service as they might be. They lack neither zeal nor means in many cases, but they do lack some one to guide their tottering footsteps. Their want of experience gives them but a restricted view of the field in which they are laboring. Their labors can not, therefore, unless directed by some one who has a sufficiently broad view of the whole field, be of any value to geology. Who will direct them? Or shall they go on piling higher their wasted energies, and find themselves when they have come to the end with the mortification of knowing that, though they have worked hard and faithfully, they have, in reality, contributed nothing to the sum of human knowledge?

If we could have some sort of co-operation, a man at work upon a particular subject would have some assurance that his field of operations would be, within all reasonable limits, left to him. As matters now stand a geologist is often obliged to mount guard over his own grounds and his own work to keep the unscrupulous and unbridled camp-followers of science from walking off with and getting the credit for the results of his labors.

Co-operation would enable each one to concentrate his efforts upon that line of work or that investigation in which he is especially interested. As matters have gone heretofore, no State survey and no man on a State survey has been able to take up any one subject in a systematic and thorough manner unless it has happened that some one group of facts has been available in his own State alone. Take any topic you may choose for a test, and you will find this to be an invariable rule.

Do the best we may, there is not one of us who may not be benefited more or less by a friendly criticism. And it is of great importance to the science that these criticisms be made before our results or observations are published. In this way we may avoid adding to that vast talus of geologic trash beneath which the science of geology is buried more and more each year. Such criticism is not possible except under conditions that enable us to know the lay of the land with reference to other geologists and to what they have done and are doing.

It should be distinctly understood from the outset that such work is to be, not subordination, but co-ordination, and above all, co-operation. The demands of scientific work do not require, and the conditions and peculiarities surrounding scientific ambition and devotion do not admit of the most successful and satisfactory work being done by machinery.

I would not by any means destroy the autonomy of local societies or of independent workers not formally connected with the public surveys. Certain independence of thought and action is essential to scientific advancement, and friendly rivalry is not only not injurious, but it is extremely helpful, and in many cases absolutely essential. I have no idea that a "perpetual motion" sort of a geological machine can be devised, or that any arrangement or adjustment of parts is possible which will entirely do away with friction.

It is scarcely possible that any device that can be made or suggested would be perfectly satisfactory, but it certainly is reasonable to expect that some system of co-operation can be devised and put into practical operation. If ever such co-operation should be brought about, several points must be kept in mind by us all. As

much latitude as possible would have to be allowed individuality. Men are not like pieces of coal to be separated and classified by sizes or by specific gravity.

Administrative methods devised for scientific work, like those of diplomacy, are often a series of compromises, and good sense must make up for the defects of any system.

No plan of co-operation can succeed if we do not all take a broad and unselfish view of science and its functions. Local talent should be utilized. It would in many cases save a good deal that now goes to pay travelling expenses, to say nothing of the importance of keeping all the geologists of the country actively interested in geological work.

Now, if geologic work can be improved by being under the nominal direction of those best fitted to direct, where are we to find our directors? The men who have done most to popularize the science of geology in this country are our professional geologists, and it is not unnatural that we should turn to them. But the teachers of a science are not necessarily the best directors of research, while they are probably in no case thoroughly conversant with the work being done by the various State surveys and by the National survey.

The direction of work over the whole country would be quite as impossible, or even more so, from the States.

The National survey, standing as it does at the head of all the geologic work done in the country, having the whole national domain as its field, and composed, as it is, of our best geologists, and having the most thorough organization, is, or should be, the natural head and director of all geological work in this country. I have no doubt that the National survey would be glad to help, in so far as it can, to unify and give useful direction to this work.

I take this ground in the face of the statement of the distinguished Director of the United States Geological Survey, who has said that "all of this scientific research under National, State, or local patronage cannot be controlled by some central authority as an army by its general, from the fact that scientific men, competent to pursue original research, are peculiarly averse to dictation and official management. Scientific men spurn authority, but seek for co-ordination."

Such a statement as this must necessarily be taken with some allowance. The function of a director or of a superior, in science at least, is not, to be sure, that of a commander ordering here and there men who must act like machines, who must have no independent opinions or plans of their own; he must rather be a helper, a man to encourage, to suggest, to fire with enthusiasm those under him, and to unify the work of the organization of which he is the head. Scientific men do not spurn authority if there is any reason for it, and as a proof of it we may cite the United States Geological Survey itself, as well as all the State Geological Surveys in this country, or, for the matter of that, in the world. The members of these surveys submit to all reasonable authority, but they are also put upon congenial work, and they are permitted to do that work pretty much in their own way. Now, why can there not be an organization of all geologists, more or less similar to this?

We may disabuse our minds of the thought that there is a probability or even a possibility of the Government monopolizing geology. It can't do it; geology belongs to the geologists, whether the Government helps carry on geologic investigations or not.

My conclusions are:—

1. That the great and valuable contributions to geologic knowledge must be made by our official surveys, for they alone have the means for producing them—for gathering the facts, giving the necessary time to philosophical thought and discussion, and for furnishing the necessary illustrations and distributing the publications.

2. That economic problems should be left, in so far as it is possible, to the State surveys, while the National survey should deal with those requiring larger means and a wider range of observations.

3. That all the working geologists of the country should be brought into official or quasi-official relations with the State and National surveys, and their efforts and skill thus utilized.

I am free to admit, however, that no plan of operation or co-operation can be devised that will work to the complete satisfaction of everybody. We sometimes have men to deal with who are not amenable to either law or reason.

In his presidential address before the American Association at Cleveland, Professor Langley compared the advance made by scientific men in their search after truth to that of a pack of hounds following a trail. Permit me to carry this simile still further. Hounds understand that it is their business to follow the game, and, when left to their own instincts and wishes, they will follow it. Now imagine a bull dog seized with the ambition to become a hunter and joining the pack of hounds. Every one knows that the bull dog will, in spite of any thing that can be done, have a fight with half a dozen, or, more likely, with the whole pack of hounds, by the time the chase is well under way.

It is not a pleasing reflection to remember that the great search after truth, in which every genuine man of science is engaged, heart and soul, is often interrupted in this same fashion by the pugnacious disposition of some companion.

Let me recapitulate some of the benefits to be derived from voluntary and cordial co-operation between all geologists and all geologic organizations in this country:

1. Geologic research being under the nominal direction of the leading investigators, would be so conducted as to be of the greatest utility to the largest number.
2. When a piece of work was done by one it would be done for all, and duplication by State surveys and by individuals and the consequent waste of energy, time, and money would cease.
3. The functions and fields of official organizations being better defined, State and National surveys and individuals could so direct their efforts as to serve the purposes of others without neglecting their own immediate aims and without infringing upon each others' grounds.
4. National and State surveys would be strengthened, and local organizations and individual effort encouraged.
5. It would give us a better geologic literature, better instruction, better geologists, and more thorough specialists.
6. And finally, we trust, it would put a stop to those oracles of science who are so ready to prophesy in its name. This ideal state of affairs may never be brought about, but it is none the less desirable that we should aim at it. For the more nearly we approximate to it the more rapid will be the progress of science, and the progress of science is the progress of civilization.

To paraphrase a recent utterance of Bishop Potter, "It would be a monstrous conception of science if any one of us were to esteem it only as a selfish weapon with which he was to carve his way to personal fame and fortune." It has often been used for just that purpose. Higher ideals will give us nobler motives.

ON CERTAIN PHENOMENA OF GROWING OLD.¹

AFTER a few introductory remarks on the choice of a subject, Dr. Minot said that he had been guided in his selection by the idea of taking a subject which would be of general interest and indicate, if possible, the new directions in which biology is developing. For this reason he had chosen the subject as announced in the title of the address. He spoke first of the law of variations as connected with the age of the living organism. When variations occur which are due simply to chance, it is found that they are distributed according to a regular curve on either side of a maximum; but when we study the variations which occur in the living organism we find that there the curve is irregular, and that there is a certain point of maximum which occurs at a definite age, and that the ascent of that curve toward the maximum is steeper upon the young side than upon the older. The speaker presented a number of examples of this taken from the age at which Harvard students enter college, from the growth of children, from the age at which maturity is attained in the female sex, from the age of mothers and the number of children which they have had at each age, and giving other examples, without, however, presenting

them in statistical form. In all of these cases the same peculiarity of the curve being steep on the young side, and less steep upon the old side, recurs. But in all these cases the maximum occurs at a comparatively early period of life. In other cases, as, for instance, when we discuss the relation of suicide to age, we find that the maximum frequency occurs at a much more advanced period, and in this case the curve becomes steep upon the old side, so that there is here a large field of statistical inquiry which is to be worked out, and there is a large amount of material which might, if properly put into shape, yield valuable results. We might study from this point of view the relation of various diseases to age, the relation of the birth of the first child to the age of the parent, of the acquisition of fame, the age of second marriage, the age at which distinguished authors have published their first book, the age of entering the United States Senate, etc. All these and other similar data might be utilized for the purpose of the biologist to study the law of variation in connection with age. At the present time there is not sufficient work done in this direction to enable us to draw any more general conclusion than that which has been presented above.

The peculiarity of the curve of variation is unquestionably due to what may be called senescence, or growing old. This senescence shows itself in the fact that toward the younger period the same range of alteration takes place as toward the older period in more advanced age. This is particularly well illustrated by a series of elaborate experiments upon guinea pigs and their growth made by the speaker. These experiments, which were interrupted by an accident which destroyed the whole stock of animals, show that the loss of vital power commences with birth, and that in order to add a given percentage to the weight of an animal a much longer period is required when it is old than when it is young. This was illustrated by statistics and diagrams. The general result may best be expressed by saying that the older an organism is, the more time it requires to produce a given change, and this indicates that there is a progressive loss of vitality. The difference between this view and the current one is that, in the speaker's opinion, there is, scientifically speaking, no period of development, but only a steady decline from birth onwards.

The speaker then turned to the second part of his subject, and discussed how far anatomical peculiarities can be found to be correlated with this progressive line of vitality. He took up the various tissues of the body, considering them one after another in their order of development, and showed that in each one of the principal tissues and organs the cells composing them exhibit the same peculiarity; namely, that in their young condition they contain only a small amount of protoplasm, and in their adult condition a very much larger amount, so that the proportion of protoplasm to the nucleus increases with the age of the organism. This fact, which can be readily verified in the case of the higher animals, finds also certain support in the development of many of the lower forms, which were also briefly discussed. Hence the conclusion that the development of protoplasm is associated with the loss of vitality, and that instead of speaking of protoplasm as the physical basis of life, we might speak of it as the physical basis of advancing decrepitude; or, since the changes involved in growing old lead to death, we might designate it as the physical cause of death. These definitions of protoplasm are too dictionary-like, and might be misleading if taken strictly, but they can at least teach us that protoplasm is by no means a simple jelly which explains in a simple manner all the phenomena of life, but it is in reality an extremely complex substance, as complex as life itself. We see in this problem of age a series of phenomena which are not especially associated with any organ or any system of organs of the body, but something which involves all parts alike. Such a study as this goes, strictly speaking, in the direction of general biology. Hitherto we have had comparative anatomy and physiology, but of general biology extremely little. The speaker expressed his belief that the future of biology would lead in this direction, and that the study of the organism as a whole would supercede in the near future to a large extent the present study of the separate organs, both in their physiological and morphological aspects.

There is a great deal to be done, for it is only in the domain of

¹ Abstract of an address before the Section of Biology of the American Association for the Advancement of Science, at Indianapolis, Ind., Aug. 20, 1890, by Charles S. Minot, vice-president of the section.

general biology that we can seek the solution of the problems of reproduction, heredity, sex, growth, variation, death, the evolution of species, and the general economy of nature. All of this phenomena are common alike to the vegetable and the animal world, and for their thorough study it is necessary that the investigator should be strictly a general biologist and not merely a botanist or zoologist. These investigations are necessarily expensive in character, since they require that the animals and plants experimented upon be kept under specific conditions for long periods of time; but, it is to be believed that the results which may be obtained will amply justify both the labor and the expense. The speaker expressed the hope that some persons who felt generously inclined to aid in scientific work might give support for this line of work, and in concluding expressed the more general hope that those persons of wealth who wish to contribute to science may feel more and more inclined to endow research, for much can be accomplished in this way than in any other for the advancement of science.

Wherever we turn we see scientific work of the highest quality delayed and even stopped for the lack of means. Every one who can rescue these opportunities from being lost, even in part, will deserve well of mankind and the acknowledgments of the association.

AMONG THE PUBLISHERS.

—California topics occupy considerable space in the September *Century*. A paper of interest, practically illustrated, is Commander C. F. Goodrich's description of "Our New Naval Guns," detailing the process of manufacture and recounting their remarkable efficiency.

—Thomas Stevens (who went to meet Stanley when the latter was coming from the interior of Africa) speaks of his article on "African River and Lake Systems" in the September *Scribner's*, as suggested by several interesting discussions of the subject while

he was in Zanzibar and Cairo—notably one with Mason Bey, one of the best authorities on African affairs, who with Prout (a name familiar to readers of the magazine) explored the White Nile in 1877. Much of the information which he gained from Mason Bey appears in Mr. Stevens's article. Professor Shaler of Harvard, in his article on "Nature and Man in America," in the same number says: "It seems to me that it is rather to the physical conditions of North America than to any primal incapacity on the part of its indigenous peoples to take on civilization, that we must attribute the failure of indigenous man within its limits to advance beyond the lowest grades of barbarism. The Indian shows us in many ways that he is an able person. We may judge any folk by their greater men, and there can be no doubt that the ablest of our American savages rank high in the intellectual scale. It is, it seems to me, to the ceaseless disturbances of nascent civilization that we owe the failure of this folk to attain to a higher grade." Apropos of railway strikes, James S. Norton, a prominent Western lawyer, says, in the same issue, "If a corporation is held to strict performance of its duty as a public servant, should not its agents, who live upon its business, be held to some account—at least for combinations made to obstruct a public service as a means to satisfy the personal grudge of a few individuals?"

—The September number of *The Forum* will contain a political essay on "Money Interests in Political Affairs," by E. L. Godkin, editor of the New York *Evening Post*, in which Mr. Godkin traces to the growth of protection the enormous and alarming increase of the influence of money in politics, which he regards as the most important political fact of our time. In the same number Senator John T. Morgan of Alabama replies to Senator Chandler's recent article on "The Federal Control of Elections." Other articles in this number will be "The Training of Teachers," by President G. Stanley Hall of Clark University; "The Christianity of the Future," by Professor John S. Blackie of Edinburgh; "The Latest Astronomical News," by Professor Charles A. Young of Princeton;

Publications received at Editor's Office, Aug. 11-23.

- BALLOU, W. R. A Compend of Equine Anatomy and Physiology. Philadelphia, Blakiston. 205 p. 12°. \$1.
 BREWER, W. H. Warren's New Physical Geography. Philadelphia, Cowperthwait. 144 p. f°. \$1.
 CRITTENDEN, E. P. The Pleroma. A Poem of the Christ. New York, Putnam. 347 p. 8°. \$2.50.
 DAY, D. T. Mineral Resources of the United States, 1888. Washington, Government. 652 p. 8°. 50 cents.
 DENTAL MIRROR. Vol. I. No. 1. m. New York, Dental Publ. Co. 16 p. f°. \$1.
 DRAGON FLIES vs. Mosquitoes. Can the Mosquito Pest be Mitigated? (The Lamborn Essays, by Working Entomologists.) New York, Appleton. 202 p. 8°. \$1.50.
 LITCHFIELD, Mary E. The Nine Worlds: Stories from Norse Mythology. Boston, Ginn. 163 p. 12°. \$1.75.
 MARCON, J. B. Bibliography of North American Paleontology in the year 1886. Washington, Smithsonian Institution. 57 p. 8°. \$1.
 POULTON, E. B. The Colours of Animals, Their Meaning and Use, Especially Considered in the Case of Insects. New York, Appleton. 360 p. 12°. \$1.75.
 SCHOFIELD, A. Health at Home Tracts, 1-12. London, Rel. Tract Soc.; New York, Revell. 192 p. 12°. 60 cents.
 SMITH, E. F. Electro-Chemical Analysis. Philadelphia, Blakiston. 116 p. 12°. \$1.
 UNIFORMED RANK ARGUS. (Published in the interests of the Uniformed Rank, Knights of Honor.) Vol. I. No. 1. m. New Orleans, C. H. Ludwig. 4 p. f°. 50 cents.
 WALKER, F. Practical Dynamo-Building for Amateurs. (Science Series.) New York, Van Nostrand. 104 p. 16°. 50 cents.

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"Protection Against Tornadoes," by Lieut. John P. Finley; as well as articles by Simon Sterne, Edward Everett Hale, James E. Murdock, M. J. Savage, and Alice E. Ives.

—The Religious Tract Society of London, believing that due care of the body is as much a Christian duty as concern for the soul, makes it an important part of its work to diffuse sound information, in a popular and readable form, in such a way as to lead people to use all the means in their power to preserve their own health and the health of those dependent upon them. With this object in view, the society has issued a neat little volume entitled "Health at Home Tracts," written by Alfred Schofield, M.D. The book is made up of a dozen tracts which had previously been issued separately, and which had been so well received by the public that their publication in the present form was determined upon. Fleming H. Revell of New York and Chicago represents the Tract Society in this country.

—It appears from a publication recently issued by the Government Statistician of New South Wales that many descriptions of gems and gem stones have been discovered in various parts of the Australian colonies, but no systematic search has been made for any but the diamond. Diamonds are found in New South Wales, Victoria, and Queensland, but only in the first-named colony have any attempts been made to work the diamond drifts. The principal diamond fields are situated at Bingera, near Inverell, in the New England district. The Government of New South Wales has, on various occasions, obtained the services of experts to report upon the fields, as well as the gems which have been from time to time extracted from them, and these reports, it is said, have generally been of an encouraging nature. The number of diamonds found in the colony to the end of 1887 is estimated at 75,000, the largest one being of 5½ carats, or 16.2 grains. The diamonds occur in old tertiary river drifts, and in the more recent drifts derived from them. The deposits are extensive, and have not yet been thoroughly prospected. The New South Wales

diamonds are harder and much whiter than the South African diamonds, and are classified on a par with the best Brazilian gems. During the year 1887 the diamond companies at Cope's Creek, near Bingera, produced about 23,000 diamonds, weighing 5,151 carats; but in 1888, owing to the severe drought which occurred, the search for diamonds had to be temporarily abandoned.

—The September number of the *New England Magazine* will be strong in agricultural articles. James Knapp Reeve tells of the advance of agricultural schools, and traces back the history of agricultural education. Another paper in this series gives an interesting treatment of the present condition of the farmer, while an article with the title "Moses in Massachusetts" cannot fail to stir up interest, especially among the readers of Henry George. The *New England Magazine* is bent upon honoring the South. The University of Georgia comes second in her series of illustrated articles on the colleges of America, and is presented by Charles Morton Strahan in this number with a series of engravings.

—In *Lippincott's Magazine* for September, Rear-Admiral Daniel Ammen, one of the chief promoters of the Nicaragua Canal scheme, contributes an article which gives a history of the inception of the project and the progress made up to date, and indicates the results which will accrue from the construction of the canal. A timely article upon "Current Concentration of Industrial Capital" is contributed by Henry Clews. He inveighs in strong terms against the present "trusts" and "combinations," and predicts their speedy downfall. He holds, however, that some form of combination is necessary to all large enterprises, and that existing State laws relating to corporate enterprises need but to be broadened in their scope, and more strictly defined in respect to their safeguards, in order to keep all combinations within proper bounds. Alfred C. Haddon gives some interesting descriptions of various dances he witnessed among the Papuans of Torres Straits, and Charles McIlvaine (Tobe Hoge) has an entertaining article upon "Superstitions about Birds."

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CHEMIST.—A Harvard Graduate and Specialist in Chemistry desires a position with some manufacturing industry, or with a practical chemist. He would make an engagement to take charge of a laboratory and lecture on Chemistry. He has had large experience in the laboratory and school-room, and can give the best of references. Address J., Harvard College, Cambridge, Mass.

WANTED.—A Lady Teacher, at present teaching in England, with 12 years of very successful experience in primary work, desires a similar position in America. An appointment in the State of Ohio, or the City of New York, preferred. Excellent references, etc. Address in July or August, L. DUNCAN, Poste Restante, Stainland, Halifax, England.

ZOOLOGIST, acquainted with the United States, graduate (Ph.D. Freiburg, B.Sc. of an English university) of many years' standing; late Fellow of an English college; late assistant to a German university, professor of Comp. Anatomy; many years a student at two German universities; pupil of Huxley, Kölliker, Semper, Wiedersheim and Weismann; author of many well-known memoirs on Comp. Anatomy and Embryology; thoroughly conversant with all branches of the subject; fond of teaching, with good testimonials, seeks a Professorship. Address "MORPHOLOGIST," Office of *SCIENCE*. Other information and a few testimonials can be seen at the Office of *SCIENCE*.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 47 Lafayette Place, New York.]

I wish to exchange Riley's Missouri Reports, either singly or full set, for Proceedings Entomological Society, Philadelphia, vols. 2 to 6 inclusive. Will also exchange Casey's Revision of the *Stenini* of N. A. and Horn's Revision of the *Tenebrionidae*. F. M. Webster, La Fayette, Ind.

I wish to exchange a fresh and complete set of Weiss and Spier's "Necropolis of Ancon" for other books, either new or second-hand, if the latter are in good condition. What have you to offer? Address Warren Watson, Kansas City, Mo.

1,000 botanical specimens representing 250 genera in exchange for those not in my collection. Send list of duplicates, and receive a similar one to choose from. Address E. E. Bogue, Orwell, Ashta Co., Ohio.

For exchange—14 volumes (1883-1889) American Journal of Science, 10 volumes (1880-1889) American Naturalist, 21 volumes (1879-1890) English Mechanic, all in good condition for binding; also, Morris Typewriter. Wants first-class microscope, camera, chemical or physical apparatus. Address, with full particulars, E. R. Chadbourne, Lewiston, Maine.

Meteorites.—Mr. George F. Kunz, 11 to 15 Union Square, New York, or Hoboken, N. J., will buy or give in exchange for whole or parts of meteorites. Whole ones, undescribed ones, and entire falls especially desired.

Wanted.—To furnish roots of *Dodecatheon Meadia*, *Sarracenia purpurea*, and other wild flowers, native of Southern Wisconsin, in quantities. D. E. Willard, Curator of Museum, Albion Academy, Albion, Wis.

I have a number of duplicates of microscopic slides, mostly botanical, which I would like to exchange for others not now in my collection. Send list of what you have to exchange and get my list. S. R. Thompson, New Wilmington, Pa.